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REVIEWS OF BOOKS.

Science and Hypothesis. By H. POINCARÉ. Translated by G. B. HALSTED, with an introduction by JOSIAH ROYCE. New York, The Science Press, 1905. — pp. xxxi, 196.

In an essay of this kind, attempting to bridge the gap between two realms of thought, one source of weakness is almost inevitable — the difficulty of acquiring independent competence in widely divergent lines of research. In this respect M. Poincaré is most fortunate, being (as Professor Royce says) "as geometer, as analyst, and as a theoretical physicist, a leader of his age," and "a great special investigator who is also a philosopher." Certain defects in his equipment are, however, quite prominent. In the first place, he lacks psychological training. I do not mean that he is ignorant of the elementary facts of psychology; for that is not the case. But of what psychological investigation means — how its results are established, in what their accuracy consists — he appears to have very little inkling. Thus we find him speaking (in connection with mathematical induction) of "the power of the mind, which knows itself capable of conceiving the indefinite repetition of the same act when once this act is possible. *The mind has a direct intuition of this power*" (p. 13; italics mine). What species of hypothesis is this?

In the second place, M. Poincaré is handicapped by the lack of a general logical theory upon which to base his special logical investigations. Thus, for example, he has no general theory of induction: "I shall not have the presumption to discuss this question after so many philosophers have vainly striven to solve it" (p. 94).

In the third place — but perhaps to some readers this will appear to be an advantage — our author has no general theory of knowledge; and he passes by the most obvious epistemological considerations without so much as a nod of recognition. At times we are led to expect more, and our disappointment is proportionately great. Certain geometrical and mechanical principles are found to be, not true indeed, but maximally convenient (pp. 39, 98); and we pass on to other matters without once recalling that for an important school of thinkers truth is no more than economy of experience. In a very brilliant section on the "Meaning of Physical Theories," M. Poincaré declares that real objects are unknowable; "nature will eternally hide from us. The true relations between these real objects are the only reality we can attain to, and the only condition is that the same relations exist between these objects as between the images by which we are forced to replace them. . . . To those who find we restrict too much the domain accessible to the scientist, I answer: These questions which we interdict to you and which you regret, are not only insoluble, they are illusory and have no meaning" (pp. 114, 116). Yet the distinction here assumed between the nature of objects and the relations between them, and the conception of identical relations subsisting between radically different terms (objects and images) raise not a moment's suspicion — despite the fact that in the historical discussion which follows the distinction shrinks to a mere recognition of relevant and irrelevant elements in hypotheses (pp. 116, 117).

In a word, the whole machinery of M. Poincaré's mind is essentially mathematical. His mode of investigation — the problems which appeal to him, the analysis to which these are subjected, the ideals of demonstrative cogency which govern the whole process — is simply that of the modern logic of mathematics, such as the phenomena of a multiplicity of algebras and geometries have called into existence. The bridge between mathematics and epistemology is almost wholly from the former side of the gap.

As the title of the work indicates, the subject of the present investigation is the place and function of hypotheses in science. The net outcome is somewhat as follows:

There exist two very sharply distinguished classes of hypotheses.[1] The one class consists of certain of the fundamental principles of the mathematical sciences; the other embraces the gradually developing results of the experimental sciences. The former are really *disguised definitions*, framed by the human mind with perfect freedom, according to its greatest convenience; like other such definitions they are purely *conventional*, and hence neither true nor false; though arising out of experience, they can be neither verified nor refuted by experience. The latter are veritable inductions, through which anticipations of experience are made possible; they spring from a tendency of the mind to seek simplicity beneath apparent complexity — a tendency for which no sufficient logical justification can be given, but without which science would be impossible.

At the outset, a certain misinterpretation — which Professor Royce apparently commits in his otherwise excellent introduction — is not unnatural; namely, that the theory of conventional hypotheses is to be conceived as a substitute for the Kantian theory of the *a priori*. But our author clearly does not so conceive it. On the contary, the latter theory is held by him in firm conjunction with the former, and his first chapter is devoted to a demonstration of its indispensability. In the course of the work, two examples of *a priori* principles are given: the formula of demonstration by 'mathematical induction' (p. 13), and the 'group'-concept (p. 53). It is the former of these that is considered in the opening chapter, and the arguments employed are worth noting for the light which they throw on the author's conception of the relation between analysis and synthesis in thought.

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[1] A third class, of which a very interesting account is given (*cf.* pp. 2, 109), but which we cannot here consider, consists of hypotheses made to facilitate calculations whose results they do not affect.

The formula of mathematical induction is as follows: Suppose, first (major premise), that if a certain theorem is true of any positive integer x, it will then be true of $x + 1$; and suppose, secondly (minor premise), that this theorem is actually true of 1; then (conclusion) it will be true of all positive integers. M. Poincaré is to prove that this is a synthetic judgment *a priori*. Its *a priori* character he infers from the combined facts of its absolute universality and obvious independence of convention. The main point of the argument lies, therefore, in showing that it is truly synthetic, *i. e.*, not reducible to an identity; or, in other words, that demonstration by mathematical induction is not reducible to syllogistic form. To prove this, M. Poincaré shows that the demonstration may be expressed in the form of an infinite series of syllogisms (p. 11), and concludes (p. 12) that the syllogism is therefore inadequate to express its true nature. He fails to perceive that the major premise — quite apart from the minor — may be transformed to read: If a theorem is true of x, it is true of every integer greater than x; when, if the minor premise be added — It is true of 1 — it follows in true syllogistic fashion that, it is true of every integer greater than 1. As for the transformation of the major premise, that may easily be justified as follows: If there be any integers greater than x of which the theorem is untrue, let y be the least of these; then the theorem is true of $y - 1$ and untrue of $(y - 1) + 1$, which is impossible; hence, if the theorem is true of x, it is true of all greater integers. The formula of mathematical induction is thus neither more synthetical nor less analytical than the concept of the number-system upon which it is based.

We pass on to the more interesting questions pertaining to the first class of hypotheses. The examples considered are: (1) The conception of a continuous number-system, (2) the geometric axioms, and (3) the principles of the classic mechanisms and of the more recent energetics. The case of continuous number need not long detain us. M. Poincaré shows in the usual fashion that this conception implies the intercalation of new elements (fractions and surds) between the elements of the system of whole numbers; and, furthermore, that these intercalated terms are mere symbols, having no meaning apart from the processes which gave rise to them and the relations in which they consequently stand. "The mind has the faculty of creating symbols, and it is thus that it has constructed the mathematical continuum, which is only a particular system of symbols" (p. 23). He does not consider the possibility that the positive integers are neither more nor less symbolical — equally destitute of meaning apart from their origin (the counting-process) and their consequent mutual relations. Measurement, he tells us, requires "the aid of a new and special convention" (p. 24). Should we not rather say that measurement of some sort is essential to the counting-process itself; and that equality of units means simply their mutual replaceability for the purposes in question? The word 'convention,' as it is here employed, means so much and so little as to call for incessant re-explanation.

The treatment of the geometric axioms is to my mind the most valuable and suggestive part of the work. That its conclusions are not entirely satisfactory is mainly due to the author's fluctuating and half-hearted adherence to the doctrine of the relativity of space and of motion. As a philosopher, he feels compelled to adhere to it; but, as a geometer, the contrary doctrine would be very convenient to him (p. 89). Like Mr. B. Russell, he takes Newton's argument for absolute motion (the polar flattening of an isolated rotating sphere) quite seriously; but instead of capitulating wholly, as the English logician feels constrained to do, he finds that "all possible solutions are equally repugnant," and hence proceeds to the conclusion that the affirmation or denial of the rotation would be equally true, but that the former hypothesis would be more convenient as permitting a simpler expression of mechanical laws. Acceptable as this conclusion is, we wonder that he does not remark that the rectilinear components of the motion of each particle of the sphere would be entirely real in the most relative of spaces — even though the axes of reference should rotate with the sphere itself. The suppositious motion is thus resolvable into a complex of real and observable strains; it is these that would explain the polar flattening, *just as they would likewise explain it if the body were surrounded by all the hosts of heaven;* and the *meaning* of the rotation, if it were asserted, would consist precisely in the reference to these real phenomena, analogous to the phenomena attending the rotation of smaller bodies upon the surface of the sphere. Where, then, would be the advantage, whether for physicist or geometer, in a theory of absolute space?

But M. Poincaré's position is more precarious than I have yet indicated. Though all motion be relative, he believes in a certain absoluteness of space itself. It is for him a real individual entity, thoroughly separate and distinct from physical objects — "objects, which," as he says, "have nothing in common with those geometry studies" (p. 99). It is on this ground that he declares: "Experiments only teach us the relations of bodies to one another; none of them bears or can bear on the relations of bodies with space, or on the mutual relations of different parts of space" (p. 60); and that is why, after all his precautions, such expressions as the "absolute orientation of the universe in space" and the "rapidity with which this orientation varies" retain such an attraction for him, even though he must add that "this orientation does not exist" (p. 89).

Over against the space of geometry, our author recognizes a 'perceptual space' with a three-fold form, visual, tactual, and motor. We have not leisure to examine the curiously dogmatic sensationalism upon which this notion of perceptual space is here founded. Suffice it to mention, as a specimen of the argument, that visual space is bidimensional because the retinal image is projected upon a surface (p. 41). Our concern is with geometrical space.

This space, we are told, belongs to no one of our representations individually, but is suggested to us by studying the laws according to which these representations succeed each other. These laws refer directly to the displacements of solid bodies; why indirectly they should characterize a further entity to be called 'space' we are not clearly informed. It is remarkable that these laws of displacements may in part be summarized by the statement that displacements form a 'group' — the general group-concept being one of the *a priori* forms of our understanding. (Of this latter proposition no proof is anywhere attempted.) However, a multiplicity of conceivable groups are each capable of satisfying the requirements. Thus, not only the Euclidean, but various forms of non-Euclidean geometry, afford a quite sufficient basis for the correlation of all possible displacements; and from the point of view of strict truth no one of these has any advantage over the others. The sole preëminence of the Euclidean geometry lies in its greater simplicity and consequently greater convenience.[1] Systems of displacements are quite conceivable — mechanicians can even make models of them — which would be more conveniently described in non-Euclidean terms; though, again, these could be perfectly, if somewhat cumbrously, described after the Euclidean fashion.

There is so much penetrating sagacity in these observations, that the element of falsity stands out with glaring distinctness. It is the superstition of absolute space. M. Poincaré does not realize that the hypothesis of (let us say) a Lobachevskian space and the compensating mechanical hypothesis which would then be necessary would simply cancel each other. It is as if one should say: Everything in the world is growing larger, and the acceleration which a given force produces in a given mass in a given time is increasing too. When one has said that, one has said nothing; it is a meaningless tautology: *a* is *a*. Now the Euclidean geometry, with its corresponding mechanics, is simply science stripped as far as possible of such tautologies. The important truth which M. Poincaré has thrown into striking relief is that geometrical and mechanical truths are correlative and mutually complementary; and this he can only express by saying that "experiments have a bearing, not on space, but on bodies!"

Thus, read in their proper connection, the Euclidean axioms are *not* mere conventional definitions, lacking truth or falsity. Comprehended as part of a larger system of relations, they represent a significant analysis of a certain aspect of experience. It is perhaps beyond the province of the reviewer to question whether truth is ever more than this. No doubt, if the ray of light, moving in an apparently homogeneous medium and the path of the apparently freely moving projectile returned upon themselves within the limits of our observation; and if an apparently taut and inelastic cord of a certain length could move freely, though its extremities were fixed — no doubt in such a case a more complex analysis would be necessary; but, in the absence of any such motive, the greater complexity would be so much sheer tautology.

We are now able to pass over very briefly our author's account of conventional hypotheses in mechanics. It is but his way of stating the truth, that force, mass, and density — yes, and space and time as well — are correlative terms without meaning apart from their mutual relations. They represent an analysis of the phenomena of moving bodies — no doubt the simplest analysis possible. This simplicity, with the attendant convenience, is all the virtue that M. Poincaré finds in them as over against an infinite number of possible analyses. But here again we must be cautious. The principles of mechanics are indeed meaningless each without the others. But together they comprehend an induction of the highest significance, which can only be obscured, n t obliterated, by the tautologies which an unnecessarily complex analysis would impose upon them.

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[1] One slip occurring in the discussion of the non-Euclidean geometries is worth mentioning. "Many defective definitions [of the straight line] have been given, but the true one is that which is implied in all the demonstrations where the straight line enters." And the author proceeds to define it as an *axis of rotation*! But has the straight line no significance in Flat-land?

A further logical confusion runs through this part of the discussion, and especially through the chapter on energetics. The principles of mechanics are said to possess two aspects: first, as "truths founded on experiment and approximately verified so far as concerns isolated systems"; and secondly, as "postulates applicable to the totality of the universe and regarded as rigorously true" (p. 98). But why this reference to the universe? And is any such distinction between inductions and postulates really called for? To be sure, the principles relate always to simple cases, that is, to isolated systems, and experiment knows no such. But surely the conception of the *limit* is entirely adequate to express all this; the ideal cases referred to by the principles are simply limiting instances. Now, though the mathematician sometimes for special purposes speaks of the infinite as a limit, he knows that it is really a very poor example — because it cannot be approached. So, unless we are wantonly to assume the universe to be finite, there is very little profit in referring mechanical principles to it. No, here as elsewhere it is as an instrument of *analysis* that the conception of limits is helpful. The phenomenon is a complex, of which our mechanics demands the analysis. Shall we say, into elements? Yes, if it be understood that the element is a category which simply expresses the restricted compass of present knowledge. Of absolute elements science knows nothing and has no need — any more than it has of a finite universe.

We must deal even more briefly with Part IV, treating of hypotheses of the second class — that is to say, inductive generalizations. We have space only for a word upon two important points. Induction, it is declared, rests upon a faith in the simplicity of natural law — "ordinarily every law is held to be simple, till the contrary is proved;" and yet in the history of science we find not only simplicity appearing behind apparent complexity, but sometimes also extreme complexity lurking behind apparent simplicity. How, then, is the faith in simplicity to be justified? And yet science cannot subsist without it. I cannot see any real difficulty here. Our faith in simplicity, so far as science depends on it, is by no means a belief that the simple is as such more probable than the complex; we should in general even incline to the contrary opinion. Our faith merely declares that an unmotived complexity is dangerous; or, more explicitly, that, other things being equal, the errors arising from a simple hypothesis will probably be less than those arising from a complex one; for even should the truth itself be exceedingly complex, the simple hypothesis is likely to be closer to it than the more complex one. But this is not paradox; it is common sense.

A chapter upon the "Calculus of Probabilities" is seriously weakened by an indefensible distinction between subjective and objective probability, the latter conception being a mere mass of direct self-contradictions. If one observes a game of chance "*a long time,*" we are told, "he *will* find that events *have taken place* in conformity with the laws of the calculus of probabilities" (p. 132); and this is the phenomenon of objective probability which M. Poincaré confesses himself helpless to explain. But the phenomenon is simply a confusion of two points of view, the uncertainty before and the certainty after the event. The conformity to the calculus, if it occurs, is a fact and not a probability — a fact, moreover, with which science is quite competent to deal.

I fear that the reader has been given but a slight notion of the exceeding interest and suggestiveness of this work. If there is much that should awaken caution, there is also a rich fund of wise and penetrating observations. Those who are least attracted by the author's conclusions may well be repaid for the reading by the impressive survey which he gives of the present state of mathematical and physical science.

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Erkenntnis und Irrtum: Skizzen zur Psychologie der Forschung. Von E. MACH. Leipzig, J. A. Barth, 1905. — pp. ix, 461.

Science is part of the human animal's reaction to its surroundings. If so, it must demonstrably serve the common end of such reactions — that of self-preservation. But the ease with which a biological function is ascribable to this or that detail of behavior varies from reaction to reaction. Obviously, we may more readily establish the service rendered by eating to the general end of living, than detect in a work on differential equations the perfecting of an arm with which to carry on the struggle for existence. To give to these higher products